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J. W. L. GLAISHER, M.A., F.R.S., President, in the Chair.

Maurice Loewy, the Observatory, Paris; and
G. Spörer, Astrophysical Observatory, Potsdam,

were balloted for and duly elected Associates of the Society.

A Reply to Mr. Neison's Strictures on Delaunay's Method of Determining the Planetary Perturbations of the Moon. By G. W. Hill.

For several years past Mr. Neison has been maintaining in the *Monthly Notices* and *Memoirs* of the Society that Delaunay's investigation of the two long period inequalities in the Moon's motion arising from the action of *Venus* is seriously defective, on account of the omission by him of a certain class of terms. In the *Monthly Notices* for last June there appears a long article by him upholding this view; to this I wish more especially to direct attention.

At the outset I may be allowed to say that all this criticism is without foundation. It appears to arise, partly from the very confused conception Mr. Neison seems to have of the nature of Delaunay's method, and partly because he fails to notice that Delaunay, after setting the degree of approximation he wishes to attain, always rigorously adheres to it. If we were obliged to admit the validity of *all* the statements in this article, an easy corollary from them would be that Lagrange's general method of the variation of arbitrary constants in the problems of mechanics was a blunder. Now, I think that no one acquainted with this method could, for a moment even, entertain such a

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proposition. Hence we may conclude there is some flaw in the reasoning of this Paper. But this must be substantiated by noticing *seriatim* the objectionable points.

In the first place, why bring forward Hansen's published values of the coefficients of these inequalities for the purpose of throwing discredit upon Delaunay's values, when their author, himself, virtually confesses he has no confidence in them, by saying he had computed them in two different ways, and found essentially different results? And, to the very end of his life, he appears never to have been able to find out whether one of these results was right, and which it was, or whether both were wrong. It would be an amusing circumstance should it turn out that the set of values, withheld from publication by Hansen, were identical with those of Delaunay.

There is some inexactitude in Mr. Neison's statement, regarding the degree of approximation adopted by Delaunay in calculating the coefficient whose argument is $8l'' - 13l'$. In this connection we note that, on account of the close proximity of the Moon to the Earth, a planet cannot produce in her motion inequalities of the same order with those it produces in the Earth, but that they are only of the order of these multiplied by the solar disturbing force; and this is as true of the indirect action as of the direct. Now, on referring to Delaunay's work, we see that he has considered, not only the term of the lowest order in each portion of the coefficient, but also multiples of this by m^2 . Hence, it is correct to say that he has considered terms of the order of the mass of *Venus* multiplied by the square of the solar disturbing force, but not those multiplied by the cube of the latter.

Mr. Neison regards the evidence adduced in his earliest Paper, as conclusively establishing the omission by Delaunay of a certain class of terms. But what was this evidence? Simply, that Hansen was at variance with Delaunay. Now, since Hansen was as much at variance with himself as he was with Delaunay, what weight ought to be attributed to this evidence? Then, Mr. Neison believes that certain discrepancies between results, obtained on the one hand by himself, and, on the other, by M. Gogou and myself, have their origin in the same cause. Now, if Hansen's investigation and also Mr. Neison's were accessible, this point could be immediately pronounced upon; but since they are not, it appears useless to speculate on the matter.

Mr. Neison says (p. 416), "He (Delaunay) substitutes the preceding value for the term in the disturbing function with the argument ζ in the differential equation and integrates." This does not correctly represent what Delaunay does. For he substitutes in the differential equation not only the term factored by $\cos \zeta$, but also the non-periodic portion of R : to wit, in the memoir of 1862, the terms

$$\frac{\mu}{2a} + m' \frac{a^2}{a'^3} \left[\frac{1}{4} + \frac{3}{8} e^2 + \frac{225}{64} \frac{e^2 n'}{n} - \left(\frac{31}{32} - \frac{971}{32} e^2 \right) \frac{n'^2}{n^2} \right],$$

and, in the memoir of 1863, the terms

$$\frac{\mu}{2a} + m' \frac{a^2}{a'^3} \left[\frac{1}{4} - \frac{3}{2} \gamma^2 + \frac{3}{8} e^2 + \frac{3}{8} e'^2 \right].$$

The terms differ in the two cases, because the degree of approximation aimed at requires the preservation of different terms with allowable neglect of all the rest. From this non-periodic portion of R results, in both cases, much the larger part of the two inequalities considered. Mr. Neison's failure to note this completely invalidates his argument on the two following pages, by which he attempts to prove the incompleteness of Delaunay's procedure. And, in this connection, it may be noted that it is not necessary that the coefficient in (38) should vanish identically in order to prove Delaunay right; it is necessary only that it should turn out of such an order of smallness as to prove that the adopted degree of approximation had been attained.

On p. 417 it is said that the coefficients B and B' "only differ by small quantities, unimportant for the present purpose." So far from this being the case, the difference B' - B constitutes one portion of the terms which Mr. Neison, all along, has been asserting were neglected by Delaunay.

That Delaunay, in treating the two *Venus* inequalities, discarded his own method, and employed the old one recommended by Poisson, is erroneously stated on p. 423. The fact is that the method followed is the same as that he had used in deriving the solar perturbations. Next, Delaunay is found fault with (p. 424) because he confines himself to calculating in R the term which has the argument of the particular inequality he is dealing with; while it is plain that there are a multitude of terms in R, having other arguments, which could contribute to the value of the coefficient sought. This is true, but Delaunay's reasons for passing by these terms are quite evident. In the first place, it must be remembered that his final expression for the inequality is a formula of substitution, which must be made, not only in the mean longitude of the Moon, but also in the equation of the centre, in the evection, variation, and in all the inequalities arising from solar action. Hence, Delaunay's method of treatment enables him to obtain, with very little additional labour, all the terms in the expression for the *true* longitude which involve the very small divisor arising from the slow motion of the argument which he is considering; and that whatever may be their arguments. And, secondly, while the terms in R, having other arguments, which would be treated by Delaunay as giving rise each to a distinct transformation, can, in a strict sense, add something to the coefficient of the inequality in the *true* longitude, practically these terms are insensible; for, although they may be of the same order, before integration, as the quantities retained, they are altogether independent of the excessively small divisor which arises from the slow motion of

the argument of the inequality. As illustrating this point, it may be remarked that, in the case of the two *Venus* inequalities in question, we get such relatively large coefficients as $16''$ and $0''.27$ only by multiplying the corresponding terms in R by factors which are about 15,000,000 in the first, and 10,000,000 in the second inequality. Hence, if there are other terms, which rigorously ought to be added to the preceding values, but which, while in other respects of the same order of smallness, have factors not much exceeding unity, it is very apparent they may be neglected.

In the next place we find Delaunay charged with neglecting every term of the solar perturbations save the term of the lowest order in the variation in calculating the proper form for R . And it is said that his development "in no sense depends on his method of transformed elements, though made to appear as if it does; nor does it differ in any way from the values hitherto employed by astronomers save in being somewhat less complete." These statements misrepresent Delaunay. He arranges under four different heads the transformations made by him, and they involve no less than 16 out of the 57 operations of his first volume, besides 4 complementary ones. And whether the amount of work in this be regarded as much or little, I have ascertained that it is precisely sufficient to obtain the degree of approximation he proposes in the coefficients B , viz., to terms involving m^2 . Carrying the approximation farther could only have afforded him terms of a higher order. It is, of course, open to Mr. Neison to say he deems this degree of approximation insufficient; and nothing can be said in opposition. But this is very different from saying Delaunay has committed errors. Again, I am not aware of the existence of any published investigation in which the degree of approximation is greater.

The reasoning Mr. Neison employs to show that Delaunay deserts, in this investigation, his own method and returns to the old method recommended by Poisson, is certainly very strange. He notes that the differential equation used has nothing in it to distinguish it from the corresponding one which Poisson would have used. But from what circumstance does this state of things arise? Simply because it is Delaunay's habit to omit, in the statement of his equations, every term which gives rise, in the final result, only to terms of a higher order than he has agreed to retain. The factors in question, in Delaunay's method, can be expressed only as infinite series; it is necessary, therefore, to cut them off at some point, and he determines this point in the way just stated. If reference is made to the same equation, in the memoir where Delaunay treats the other *Venus* inequality, it will be found to be duly distinguished by the presence of additional terms, Delaunay writing as many as are just sufficient for his purpose.

Mr. Neison next notices two assumptions, which he says have been made by Delaunay in his integration.

The first is that the factor $\frac{2}{an}$, which multiplies $\frac{dR}{dt}$, is treated as if it were constant. But here he forgets that, with Delaunay, at this stage of the work, the symbols a , e , γ , l , g , and h , denote quantities which have no solar perturbations; and that, consequently, the deviation of $\frac{2}{an}$ from a constant has the mass of the planet as a factor. Thus, as $\frac{dR}{dl}$ already has this factor, the additional terms, which would in this manner arise, would have the square of the mass of the planet as factor; these, as all other investigators, Delaunay expressly neglects.

With regard to the second assumption, in reference to which Mr. Neison makes what he thinks his chief point against Delaunay, let us consider what is the essential difference between Delaunay's method and that employed by the earlier investigators. Delaunay said to himself, Do not let us go back to the elements of the Keplerian ellipse every time we have to consider the action of a new force on the Moon, but let us determine our new wave of motion in such a way that it may be superposed on the curve which the Moon would describe under the action of all the forces previously considered, instead of on the Keplerian ellipse. At any stage of progress, in expressing the Moon's co-ordinates, there must, of necessity, appear in them six arbitrary constants which have been introduced by integration. Let us take these as variables, instead of the six elements of the Keplerian ellipse. This course demands that the differential equations employed by the earlier investigators should be somewhat modified. The modification appears as a change in the values of the quantities which Poisson denoted generally by the symbol $[a, b]$. Now, just as it would be absurd to maintain that the elements of the Keplerian ellipse suffer perturbations from the action of a centrobaric Earth, so it is absurd to maintain that the quantities a , e , γ , l , g and h , employed by Delaunay after he has got through with the solar perturbations and has arrived at the treatment of the planetary perturbations, and which are the elements of the curve which would be described by the Moon under the combined action of the Earth and Sun, suffer perturbations from the latter body. Yet Mr. Neison's argument, when divested of its obscurities, is seen to be nothing more or less than a plea that these quantities do suffer perturbations from the Sun.

To make the matter plainer, let us suppose that Delaunay, groping about in the dark, had fallen upon the Poissonian equations, and, thinking them to be his own, had used them as such; and, moreover, on making his substitutions, had made them only in the elliptic portion of the co-ordinates. Then he would have committed the very error Mr. Neison lays to his charge. But since he uses equations suitably modified to the new signification of the quantities a , e , &c., and, moreover, makes his substitutions in the complete expressions for the

Moon's co-ordinates, and not in the elliptic portion only, as the earlier investigators do, is it not plain that, by these two modifications, he obtains terms which he would not have obtained in the former supposed case? Now these terms, in sum, are precisely equivalent to those Mr. Neison accuses him of neglecting by omitting to include R''' in his disturbing function. Thus it is seen that Delaunay takes account of R''' in an indirect manner, the peculiar nature of his method absolving him from considering the terms arising from R''' as a separate class.

Perhaps the matter will be clearer still if we say that, just as in determining the solar perturbations we have no class of terms of the order of the product of the mass of the Earth by the mass of the Sun, simply because the Earth's action is considered as the principal force, so when we come to treat the planetary perturbations by Delaunay's method, there is no special class of terms of the order of the product of the Sun's mass by the planet's mass, for the reason that here the combined actions of the Earth and Sun are regarded as forming the principal force.

Next we must not pass over without notice the quite erroneous method Mr. Neison proposes (pp. 430, 431) for getting the proper expressions for the Poissonian quantities [a , b]; viz., by substituting for the elements in the expressions proper to the older form of the differential equations their complete values as functions of the time, and then neglecting all the periodic terms. It is very certain this procedure will not give the same values as Delaunay has, who obtains them by taking the partial derivatives of a , e , and γ with respect to the elements L , G , and H , which are the conjugates of l , g , and h .

Mr. Neison is not content with what he has already said to establish the serious imperfection of Delaunay's method, but fortifies himself in the belief of it by a new line of argument (pp. 432-437), where he gives his conception of the essential nature of Delaunay's transformations. But his argument is fatally vitiated because he will have it that the transformations in question are rigorously linear in their operation. Thus, to illustrate, suppose Delaunay has

Operation 1.

Replace a_0 by $a_1 + f_1(a_1, e, \&c.)$

Operation 2.

Replace a_1 by $a_2 + f_2(a_2, e_2, \&c.)$

(I use the subscripts, which Delaunay has not, that my meaning may be clear.) According to Mr. Neison's way of looking at things, these two operations are equivalent to

Replace a_0 by $a_2 + f_1(a_2, e_2, \&c.) + f_2(a_2, e_2, \&c.)$

Thus he fails to see that Delaunay intends the a_1 , under the functional sign f_1 , to be eliminated by the substitution of Operation 2, as well as the a_1 which is outside of it. In consequence he misses all the terms which are of the order of the product of f_1 by f_2 .

Now, suppose that f_1 belongs to an operation which is concerned with solar perturbations, and f_2 to one concerned with planetary perturbations. Then Mr. Neison, by his erroneous interpretation of Delaunay's processes, fails to get some terms of the order of the product of the masses of the Sun and planet, which, nevertheless, Delaunay has. Now, these are the very terms Delaunay is accused of neglecting. And, what is sufficiently singular, Mr. Neison appears to regard the symbols a , e , &c., which are under the functional signs f_1 , f_2 , &c., as having everywhere throughout the whole series of operations the same signification, and as being absolute constants; so that, for him, all the f 's are explicit functions of the time.

There is another way in which Mr. Neison's error may be illustrated. Suppose we write one of the differential equations of the Moon's motion in rectangular co-ordinates, thus

$$\frac{d^2x}{dt^2} - \frac{d\varpi_0}{dx} = \frac{dR^{(0)}}{dx} + e' \frac{dR^{(1)}}{dx} + e'^2 \frac{dR^{(2)}}{dx} + \dots + \beta \frac{dR_0}{dx} + m'' \frac{dR_1}{dx} + \&c.,$$

where ϖ_0 denotes the potential of the force exerted by a centrobaric Earth; and the portion of the disturbing function due to solar action has been broken into a number of parts $R^{(0)}$, $e'R^{(1)}$, $e'^2R^{(2)}$, &c., severally proportional to the various powers of the solar eccentricity e' ; and βR_0 is the portion due to the figure of the Earth, β being a constant which measures the deviation of the Earth from a centrobaric body; in fine, $m''R_1$, is the portion due to the action of a planet whose mass is m'' . Then Delaunay's way of proceeding is very similar to this: he first ascertains what would be the expressions for the Moon's co-ordinates were $R^{(0)}$ the complete disturbing function, by making variable the a , e , γ , l , g , and h which appear in the elliptic formulæ; he then transposes $R^{(0)}$ over to the left member of the equation, and the potential of the principal force is now no longer ϖ_0 but $\varpi_0 + R^{(0)}$; he then proceeds to treat $e'R^{(1)}$ as if it alone constituted the whole of the disturbing function, using the elements a , e , γ , l , g , and h , which stand in his last expressions for the co-ordinates as variables, not those which belong to the elliptic expressions. When this is done, $e'R^{(1)}$ is transferred to the left member, and the potential of the principal force is now $\varpi_0 + R^{(0)} + e'R^{(1)}$, and the work is continued as before.

Now, Mr. Neison admits the legitimacy of all this as long as we are dealing with the portions of the disturbing function which arise from solar action; but says that, the moment we arrive at the term $m''R_1$, all changes. Then certain ghosts, as it were, of the portions $R^{(0)}$, $e'R^{(1)}$, &c., unbidden return to the

right member and trouble the portion $m''R_1$. Thus we have the strange spectacle of forces figuring at once as principal and as disturbing. Mr. Stockwell made a precisely similar objection to my elaboration of the inequalities due to the figure of the Earth, which was disposed of by Prof. Adams in a single sentence.

If all this be true, what becomes of the assertion, often reiterated, that, when the differential equations are written down, all the rest is a pure question of analysis? On Mr. Neison's and Mr. Stockwell's view, the analyst, who does the integrating, needs an astronomical or mechanical prompter at his elbow to inform him of the exact physical import of the constants β or m'' , otherwise he will infallibly go wrong.

Washington.

1886, Oct. 14.

On Kepler's Problem. By Robert Bryant, B.A., B.Sc.

The solutions of this problem are usually given with a view of obtaining the eccentric anomaly directly from a series expanded in powers of the eccentricity, the application of which is in general very laborious.

The use of the differential formula

$$\frac{dE}{dM} = \frac{1}{1 - e \cos E},$$

when an approximate value of E in a planetary orbit is known, involves less labour, while the results converge with great rapidity.

The two following series are given as readily offering the required approximate value of E in a planetary orbit, when one application of the differential formula usually gives the eccentric anomaly with sufficient accuracy. The series are so simple and so readily present themselves that they have probably been given before, but I have not met with them.

We have $M=E-e \sin E$ with M and e , given to find E .

Lagrange's theorem may be concisely expressed thus :

If

$$y = z + x\phi(y), \text{ then } f(y) = \sum_{n=0}^{n=\infty} \frac{x^n}{n!} \frac{d^{n-1}}{dz^{n-1}} \{\overline{\phi(z)}^n f'(z)\},$$

the interpretation of which when $n=0$ is obvious.

If we expand $\sin E$ and $\cos E$, we obtain

$$\begin{aligned} \sin E &= \sin M + e \sin M \cos M + \frac{e^2}{2} \sin M (2 - 3 \sin^2 M) + \frac{e^3}{3} \sin M \cos M (3 - 8 \sin^2 M) \\ &\quad + \frac{e^4}{24} \sin M (24 - 136 \sin M + 125 \sin^4 M) + \dots \\ \cos E &= \cos M - e \sin^2 M - \frac{3e^2}{2} \sin^2 M \cos M - \frac{2e^3}{3} \sin^2 M (3 - 4 \sin^2 M) \\ &\quad + \frac{5e^4}{24} \sin^2 M \cos M (12 - 4 \sin^2 M) + \dots \end{aligned}$$